II. *On the Structure and Uses of the Spleen.* *By* Everard Home, *Esq. F. R. S.*

Read November 26, 1807.

In bringing forward a fact of so much importance, as a communication between the cardiac portion of the stomach and the circulation of the blood, through the medium of the spleen, I shall not take up the time of the Society by offering any preliminary observations, but state the circumstances which led to the discovery, and the experiments by which the different facts have been ascertained.

During the investigation of the functions of the stomach, (in which I have been lately engaged,) it was found that while digestion is going on, there is a separation between the cardiac and pyloric portions, either by means of a permanent or muscular contraction. This fact placed the process of digestion in a new light, and led me to consider in what way the quantities of different liquors, which are so often taken into the stomach, can be prevented from being mixed with the half digested food, and interfering with the formation of chyle.

Pursuing this enquiry, I found that the fluids are principally contained in the cardiac portion, and the food that has reached the pyloric portion is usually of one uniform consistence, so that the fluids beyond what are necessary for digestion would appear to be carried out of the stomach, without ever reaching

so far as the pylorus. To ascertain the truth of this opinion is the object of the present Paper.

The lymphatic vessels of the stomach are numerous, but they are equally or more so in the other viscera. Many circumstances appeared to render it probable that the spleen is the route by which liquids are conveyed. The more I considered the subject, new reasons in favour of this opinion crowded on my mind, so as almost to enforce conviction, and made me set about devising various methods, by which its truth or falsehood might be established.

The first point to be decided was, whether the liquids received into the stomach do escape in any considerable quantity, when prevented from passing out at the pylorus.

This was ascertained by the following experiment, made October 31, 1807, with the assistance of Mr. BRODIE, Mr. W. BRANDE, and Mr. CLIFT.

The pylorus of a small dog was secured by a ligature, and a few minutes afterwards five ounces by measure, of an infusion of indigo in water, of the temperature of the atmosphere, were injected by the mouth into the stomach. At the end of half an hour the dog became sick, and brought up by vomiting 2 ounces of a nearly colourless fluid. The dog was immediately killed, and the different parts were examined. The pylorus was found completely secured by the ligature, so that nothing could pass in that direction The pyloric portion of the stomach was found empty and contracted; the cardiac portion contained about two ounces of solid contents, enveloped in a gelatinous substance, and one ounce of water with little or no colour, the indigo being completely separated from it, and spread over the surface of the internal mem-

brane. Of the five ounces of water thrown into the stomach, two were brought up by vomiting, and one only remained; two ounces had therefore escaped in the course of half an hour. As the stomach contained two ounces of solid food at the time the experiment was made, it is reasonable to suppose that there was also some liquid in it, and in this case the whole quantity that escaped must have exceeded two ounces. On examining the external covering of the stomach, and along the course of the vasa brevia, where the absorbents usually pass, none were discovered, so that these vessels were not at that time carrying any liquid.

The spleen was turgid, unusually large, and its external surface very irregular; when cut into, small cells were every where met with containing a watery fluid, and occupying a considerable portion of its substance. This appearance, which I had never seen before, made me enquire, if it had been taken notice of by others, and endeavour to ascertain the circumstances under which it is produced. The following statement contains the information which I have received on this subject.

MALPIGHI appears to be the first anatomist, who had any particular knowledge of the structure of the spleen. He describes its capsule, and a network which pervades every part of the substance. He mentions a number of small glands, which are hollow, and surrounded by arterial zones, but he had never been able to trace any venal branches into them. He believed that there was a cellular structure in the spleen containing red blood, interposed between the arteries and veins; this led him to adopt a theory that the network was muscular, and by

its action propelled the blood, so that there was a systole and diastole in the spleen, as in the heart.

STUKELY, in his Gulstonian lecture, has very closely copied MALPIGHI, without giving any additional information.

CUVIER, the latest writer on this subject, in his *Leçons d'Anatomie comparée*, corrects the error of MALPIGHI respecting the nature of the network, which he states to be composed of elastic ligament, and says that there are small corpuscles, whose use is unknown, and which disappear when the blood vessels are minutely injected.

In the course of the present investigation, I have examined the spleen after death, under the ordinary circumstances, and have found the appearances described by CUVIER. I have also examined it frequently immediately after the stomach had received unusual quantities of liquids, and in that state have found invariably, that the corpuscles of CUVIER, which were the glands of MALPIGHI, are distinct cells, containing a fluid, which escapes when the cells are punctured, and renders their membranous coat visible, so that it would appear that the distension of these cells is connected with the state of the stomach, and therefore only takes place occasionally; and that the elastic capsule by which the spleen is surrounded adapts the organ to these changes in its volume.

On examining further into the structure of the spleen, in which I have been materially assisted by Mr. BRODIE, the following facts have been ascertained.

In the spleen of the bullock, horse, and hog, the cells, when the arteries and veins are injected with coloured size, are seen to have numerous arterial branches ramifying in their coats,

but no venal ones, which confirms the statement of MALPIGHI; and when the cells are empty and contracted, and the blood-vessels filled to a great degree of minuteness, the appearance of cells is entirely lost, as stated by CUVIER.

When the cells were in a distended state, their cavities in a great many instances were very distinct, having been laid open in making a section of the spleen. The intermediate parts of the spleen are but sparingly supplied with arterial branches, and the smaller ones do not appear to have any particular distribution.

When the veins only are injected their branches appear more numerous, and larger than those of the arteries, making the whole substance of the spleen of a red colour. They appear to arise from the outside of the cells going off at right angles to their circumference, like radii. Where the injection has not been very minute, they are seen to arise at so many points of the capsule; but where the injection has got into smaller branches, their number is so much increased that they appear to form plexuses round the cells.

The trunk of the splenic vein, compared with that of the artery, when both are filled with wax, is found to be in the proportion of five to one in its size. This was ascertained both by an accurate measurement of their diameters, and by weighing half an inch in length of each in a very nice balance; the disproportion between them is greater, than between corresponding veins and arteries, in other parts of the body.

Having acquired this knowledge of the internal structure of the spleen, I made the following experiment with a decoction of madder. This substance was employed, from the animals who feed on it having their bones tinged red, so that there can be no doubt of its colouring matter being carried

into the circulation of the blood. I was much disappointed on seeing the colour of the decoction, which, instead of being a bright red (the tinge communicated to the bones), was of a dirty brown. The same gentlemen assisted me, as in the former experiment.

Nov. 8, 1807, seven ounces of a strong decoction of madder were injected into the stomach of a dog, immediately after the pylorus had been secured. At this time the dog voided some urine, which was limpid and colourless. In 42 minutes, 2 ounces of a yellowish fluid were brought up by vomiting. In 18 minutes more the dog vomited again; what came up proved to consist of $3\frac{1}{2}$ ounces of solid matter, and 3 ounces of liquid. In 15 minutes afterwards, 5 ounces of the decoction were injected, which remained quietly on the stomach for two hours and a quarter, at the end of which period the dog was killed. In the act of dying he made water, in the quantity of two ounces, of a dark muddy colour. This was saved, and afterwards compared with the remaining liquid in the stomach, which it exactly resembled. On examining the connections between the stomach and spleen, none of the absorbent vessels were apparent, more than in the former experiment. The pyloric portion of the stomach contained about two ounces of half digested food, but no liquid. The cardiac portion contained four ounces of liquid, and half an ounce of solid food, so that the act of vomiting, which appeared, at the time, a sufficient exertion to have completely emptied the stomach, had brought up no part of the contents of the pyloric portion, and had not even completely emptied the cardiac portion. In this experiment, without making allowance for any liquid in the stomach, prior to the decoction of madder being injected, one-fourth part of the quantity thrown in had escaped.

The cells of the spleen were more distinctly seen than in the former experiment, particularly at the great end.

Although there was every reason to believe that the colouring matter of the madder had been conveyed into the urinary bladder, yet so muddy and indistinct was the colour, that it was by no means completely ascertained. I therefore resolved in my future experiments, to make use of some colouring substance, the presence of which could be detected in a very diluted state, by means of a chemical test; and I requested Mr. W. BRANDE, of whose assistance I have before availed myself, to point out the substances best fitted for this purpose. He immediately suggested that rhubarb was a substance which he had made use of as a test to ascertain the presence of alkali, and therefore had no doubt that the caustic alkali would prove a test of rhubarb. This substance has also another advantage; it is well known to pass very readily by the kidneys, without being decomposed.

The following are the results of experiments made with rhubarb, to ascertain the best modes of detecting it in the urine and blood, and the time it takes to pass from the stomach to the urinary bladder.

Five drops of tincture of rhubarb added to 3 ounces of water, are found to strike an orange tint when the test is added, which does not take place when the rhubarb is more diluted.

Six drops of tincture of rhubarb, added to three ounces of serum, are readily detected by the eye, but the colour is not heightened by applying the test; the alkali contained in the serum, being sufficient to strike as bright a tint, as that quantity of rhubarb can receive from the addition of alkali.

When tincture of rhubarb is mixed with blood just taken from the arm, its colouring matter is afterwards found both in the serum and in the coagulum.

When blood is drawn from the arm of a person, who has taken rhubarb in sufficient quantity to affect the urine, the serum is found to have a slight tinge from it, equal to that, which one drop of tincture of rhubarb gives to half an ounce of serum when added to it.

Half an ounce of tincture of rhubarb, diluted in $1\frac{1}{2}$ ounce of water, taken in the interval between meals, did not pass off by urine in less than an hour, and even then was not in sufficient quantity to be discovered, till the test was applied.

The same quantity was taken immediately before a breakfast consisting of tea. In 17 minutes, half an ounce of urine was voided, which when tested had a light tinge. In 30 minutes another half ounce was made, in which the tinge was stronger; and in 41 minutes a third half ounce was made, in which it was very deep. In an hour and ten minutes 7 ounces were voided, in which the tinge of rhubarb was very weak, and in two hours twelve ounces were voided, in which it was hardly perceptible.

In $6\frac{1}{2}$ hours the rhubarb acted on the bowels, and gave a decided tinge to the fæces; the urine made at the same time had a much stronger tinge, than what was voided at one hour and ten minutes.

In this experiment, the rhubarb appeared to have escaped from the cardiac portion of the stomach; and in two hours ceased to pass through that channel; but was afterwards carried into the system from the intestines, and again appeared in the urine.

This experiment was repeated on another person; the rhubarb was detected in the urine in 20 minutes. In 2 hours the tinge became very faint; in 5 hours it was scarcely perceptible; in seven hours the rhubarb acted on the bowels; and the urine made after that period, became again as highly tinged as at first.

It was suggested by a chemical friend, that the prussiate of potash might be a better substance than rhubarb, for the present experiments, since the solution of one quarter of a grain in two ounces of water, becomes of a blue colour on the addition of the acidulous muriate of iron.

To determine this point, one quarter of a grain was dissolved in two ounces of serum, but no blue colour was produced by the addition of the test, nor did this effect take place till the quantity of the prussiate was encreased to a grain; so that minute quantities of the prussiate of potash, or at least of the prussic acid, may exist in the blood, without being detected by adding solution of iron.

The effects of rhubarb on the urine, and the different parts of the blood having been thus ascertained, a third experiment was made, in which that substance was employed, and I had the assistance of the same gentlemen as in the others.

On November 17, 1807, at 35 minutes past 11 o'clock, five drams of a mixture of tincture of rhubarb and water, in the proportion of a dram to an ounce, were injected into the stomach of a dog, whose pylorus was secured. At 20 minutes past 1, two ounces of fluid were brought up by vomiting: ten minutes afterwards, another ounce of the mixture was injected, as were nine drams more at $\frac{1}{2}$ past 4 o'clock. The two last portions were retained, and at 8 o'clock in the evening the dog was killed.

On examining the parts after death, the pylorus was found to be completely secured; the stomach contained about two ounces of fluid; none of the absorbent vessels passing from its great curvature were in a distended state so as to be rendered visible. The spleen was turgid as in the former experiment, and the urinary bladder full of urine.

This urine tested by the alkali, received a deeper tinge of rhubarb than the human urine, after rhubarb had been taken three hours by the mouth, and in other respects resembled it.

When the spleen was cut into, the cells were particularly large and distinct. A portion of it was then macerated in two drams of water for ten minutes in a glass vial. All the parts were exposed to the water, by its being divided in all directions. The water thus impregnated was strained off and tested by the alkali, and immediately the reddish brown colour was produced in the centre, and no where else, but in less than a minute it began to diffuse itself, and extended over the whole.

A similar portion of the liver was treated in the same way, and the alkali was added to the strained liquor, but no change took place in it whatever.

In this experiment the rhubarb was detected in the juices of the spleen as well as in the urine; and as there was no appearance of it in the liver, it could not have arrived there through the medium of the common absorbents carrying it into the thoracic duct, and afterwards into the circulation of the blood.

The discovery of this fact I consider to be of sufficient importance to be announced to the Society, that when it is thus made public, I may be at liberty more openly, and on a more extensive scale of experiments, to prosecute the enquiry.